

Jane McLaughlin

Biographical Statement

Jane McLaughlin was born in Woburn, MA, on August 2, 1931. She earned her B.S. in biology at Trinity College of Vermont in 1952 and, that same year, began working in the laboratory of Albert Szent-Györgyi at the Marine Biological Laboratory (MBL), Woods Hole, MA. Ms. McLaughlin continued to work on a variety of projects as Szent-Györgyi's research interests evolved over the next 34 years. Shortly after Szent-Györgyi died in October 1986, Ms. McLaughlin moved to the laboratory of Raquel Sussman (Molecular Biology) at Woods Hole (1987-1988). From 1989-1993 she worked in the laboratory of Lionel Jaffe (Developmental Biology), also at Woods Hole. For the next two years (1994-1996), Ms. McLaughlin worked at the National Institutes of Health (NIH)-National Vibrating Probe Facility, Peter J. S. Smith, Director (Woods Hole), and from 1996 until her retirement in 2002 she worked at the NIH-BioCurrents Research Center, Peter J. S. Smith, Director. After her retirement Ms. McLaughlin continued to reside in Woods Hole, where she had lived since 1952.

In her fifty-year career at Woods Hole, Ms. McLaughlin authored or coauthored forty-six scientific articles. She also is author or co-author of two books on Woods Hole and New England history, and has published numerous articles on those subjects. Her awards include the Trinity College of Vermont Outstanding Alumna Award for Service (1979); the Marian Medal from the Bishop of the Diocese of Fall River, MA, for Outstanding Service (1982); and the Albert Szent-Györgyi Award for Service to Science and Mankind (1983). She is a member of the American Association of the Advancement of Science and a Member of the Corporation, Marine Biological Laboratory, Woods Hole. She also has served on the board of directors of the Woods Hole Historical Collection.

Interview Synopsis

Ms. McLaughlin describes the location and organization of Szent-Gyorgyi's lab at Woods Hole, MA from her arrival there in 1952 until Szent-Gyorgyi's death in 1986. An expert in the local history of Woods Hole, Ms. McLaughlin, like Albert Szent Gyorgyi himself, was a year-round resident and thus is able to offer keen insights into the seasonal aspects of the Marine Biological Laboratory's activities, the MBL's relationship with Szent-Gyorgyi and his lab, and the evolution of Szent-Gyorgyi's research interests over the years. She reviews a typical day in the lab, with an emphasis on Szent-Gyorgyi's work habits, his schedule, and his research and teaching style. For instance, she describes his penchant for using color reactions in test tubes as a way of dramatizing various biomolecular or submolecular phenomena. Ms. McLaughlin also covers the social aspects of life at Woods Hole, highlighting some of the many renowned scientists who visited Szent-Gyorgyi and stayed in his home, Seven Winds, on Penzance Point. Szent-Gyorgyi's need for research funds was almost always of great concern to him, she notes, though it was not a worry that he visited upon those who worked daily with him. Ms. McLaughlin sums up Szent-Gyorgyi's legacy as "the broadness of his thinking," that is, the way his theorizing embraced numerous disciplines such as quantum physics and molecular biology. She concludes that it may yet be too early to assess or to judge the ultimate value of this legacy.

**National Library of Medicine
Interview with Jane McLaughlin
Conducted on August 20, 2004, by Adrian Kinnane**

AK: Let's start in the beginning. You came to Woods Hole in 1952, I believe?

JM: Yes.

AK: Tell me something about the circumstances, how you arrived there?

JM: I had just graduated from Trinity College of Vermont, and my biology professor there had been so enthusiastic about the Marine Biological Laboratory at Woods Hole. She and her husband both came to MBL Woods Hole for many years in the summertime to do research. I thought, "I must go to Woods Hole and see what it's all about, and if I don't go immediately, I might get wound up in something else and not get there." So I came and in the course of the summer, met Andrew Szent-Györgyi and his wife, Eva. Eva introduced me to Albert and the rest of the family. Andrew and others in the lab would pass their manuscripts to me to read for my own information, and to help with a little bit of editing and so on. Anyway, along about August, Prof Szent-Györgyi offered me a two-year fellowship, and . . .

AK: That was the beginning.

JM: That was the beginning, yes.

AK: So your first contact was with Andrew, Albert's cousin.

JM: Yes, and during the summer I was included in gatherings, parties, picnics. I went to the lectures and so on.

AK: Do you recall your first meeting with Albert?

JM: I don't recall it specifically, but I think it was probably during a beach party, and probably at a beach party on Penzance, outside of his summer cottage.

AK: What were your first impressions of him?

JM: Well, I thought he was very interesting, of course, and he seemed to have some interesting ideas. At that point he was just beginning to think about things other than muscle. He was sort of at the end of his specifically muscle work and was looking into examining the sub-molecular. Not just the molecular, but the sub-molecular.

AK: He was just on the verge of going into a new way of approaching . . .

JM: Yes.

AK: You worked with him for thirty-four years.

JM: Thirty-four years. '52 to '86, when he died.

AK: Yes, that's a lot of work.

JM: Oh, it seemed to slip by pretty quickly.

AK: We'll come back to that, but let me get a sense of how he and his laboratory fit into Woods Hole, organizationally, administratively. When he first arrived, what were his funding sources?

JM: I don't know. I think he did get something from the Rockefeller Foundation when he first arrived. That was in '47 and a number of his team from Hungary came over. I can tell you there was never very much in the way of funding for the lab, but in those days you could live inexpensively and modestly, as everybody did. And so he managed to have things move along with not a great amount of money, by any means. But the post docs and the team that he brought over, of course, started getting themselves situated in different academic situations and so on. But Szent-Györgyi's lab was the first year-round lab that the MBL had, and so that was an important change for the MBL.

AK: I hadn't realized that. So his lab was the first one to operate . . .

JM: Year round.

AK: Twelve months a year.

JM: Within the MBL, yes.

AK: Within the Marine Biological Laboratory.

JM: Yes.

AK: When you joined him in '52, was his lab funded by Armour? Did he have the Armour grant?

JM: I know that he had some funding from them, but I don't know what the periods were, Adrian, and I really don't know a great deal about the funding sources. I was never concerned with that. As you know, he was not inclined to the federal grants, but somehow or other one managed. That was it.

AK: Right. I know that over the years funding was a great concern to him, partly because he had a problem with the grant system in the United States. How much of that filtered down to the people who were working with him, for him, or did he try to insulate them from that?

JM: Well, I can only speak for myself and I would say I was insulated from that. I think some of the other more senior people would have a better fix on that than I could. You said that that was an area that you were interested in delving into, but that was one area that I didn't get involved in. He took care of it in one way or another.

AK: The Institute for Muscle Research, his laboratory was not always called that.

JM: No. It was generally called that, probably up to the late '60s or '70s, early '70s, but then when the National Foundation for Cancer Research was founded, they wanted the lab to bear that name. So there was never any official beginning and end to these things, but they just sort of drifted into them.

AK: How was the lab known in the last ten or fifteen years of his work? Was there a name?

JM: Well, we—I think I probably generally referred to it as Szent-Györgyi's lab.

AK: Szent-Györgyi's lab or the Szent-Györgyi Lab.

JM: You know, that seemed to be the best description, the more informative description, instead of having to define these other things for someone who might not know.

AK: Right. Now, in terms of the physical plant, the lab, Szent-Györgyi's lab was in this building, the Lillie Building?

JM: It was in the Lillie Building, yes.

AK: Up on the third floor, was it?

JM: I think the original space his group had was on the third floor, he had that space all the time that he was here, and when I came, he had taken a couple of the smaller rooms on the second floor. Andrew's wife, Eve, was working in one of those rooms, and I was working in the other one with Szent-Györgyi, and the original muscle people were up on the third floor in the room that overlooked the sundial, the one on the left, if you're facing south. Then later, he got the room on the other side of the hallway, front facing, overlooking the sundial there and we moved upstairs. There were also other smaller rooms at times, depending on how many people there were and just what kind of work they were doing and so on. Some of the heavy equipment was kept in rooms on the basement level. Different equipment according to the evolution of the thinking.

AK: Right, I'm trying to get a sense of the growth of his lab over the years, how much space it—did it grow a lot?

JM: Well, yes. Yes, it did, and he also had some other little buildings. He had a little building built in which the larger quantity extractions could be done, and he also had space over in what is now the area of the Marine Resources building. We had an animal house up there for mice and rats. So, yes, it spread quite a bit.

AK: U-hmm. Well, now, between the time you arrived and today, of course, I imagine Woods Hole itself has gotten to be a lot bigger.

JM: When I came to Woods Hole in 1952, the total population of the town of the Falmouth was about nine thousand. Today you have ten thousand running in the road race that runs in August. There's been quite a change.

AK: Yes.

JM: There were still the early wooden buildings for the MBL, smaller lab buildings and lecture halls and so on. The Oceanographic only had its original building at that time, the Bigelow Building. Then they built the Navy building, which they called the Smith Building and the Redfield and then they've expanded out of the village. So the whole of the science community has grown considerably and the fisheries, now known as NOAA, N-O-A-A, was the first of the institutions in town and that was something that Spencer Baird had introduced. He was interested in setting up a lab and he went up and down the coastline looking for an appropriate place, and he finally settled on Woods Hole because

it had this rushing water and there were no rivers flowing in to dilute the salinity, to pollute. At that time it was relatively quiet.

Now, this is back in the late 1800s, and there were not the pollution problems and so on. So the fisheries was the first foot in Woods Hole and then the MBL, and then it was Doctor Lillie, for whom this building is named, and others who recommended, I think it was to the National Academy, that an institution for ocean studies be developed. It was Lillie who was instrumental in the Oceanographic being formed.

AK: I have the impression that Szent-Györgyi's lab operated either autonomously or semi-autonomously and that the MBL has a president and director?

JM: The MBL at that time did not have a resident director. It would be the president who would come from time-to-time when there were particular issues to be dealt with. But the MBL had a wonderful business manager, Homer Smith, and he knew how to handle people and he was well organized. He was not the first of them, but during most of the period when I've been here, he managed the MBL. He was very welcoming and he would handle requests of Szent-Györgyi to the extent that they could be, but he would do his best to accommodate a need. It wasn't until probably the late '60s and the early '70s that they started having more of a hierarchy and a resident director and so on.

I have always felt privately that it was probably good for the institution overall to have this structural change, but one of the appealing things to a person like Szent-Györgyi was that he could go to the lab and he could work and he didn't have committee meetings coming at him every time he turned around, and he could focus on his work. So by the same token, as labs began to be year-round within the MBL, then the grants would go to that person by way of the MBL. Whereas, with the sort of funding that Prof Szent-Györgyi had, a fee would be paid. You see, the funding didn't go through the MBL, but expenses would be paid and a fee for the use of the space would be paid.

AK: Right. For example, by the National Foundation for Cancer Research?

JM: Yes, yes. So that was the distinction between those two funding approaches.

AK: So he really was pretty autonomous. His funding didn't come through the MBL, but his funding source paid like a rent fee or something.

JM: Something of that order.

AK: Uh-huh. Were relations between Szent-Györgyi's lab and the MBL mostly smooth or were there some rough . . .

JM: Oh, I would say mostly smooth.

AK: From what you told me, he'd been here twenty-five years or more before they even had a hierarchy.

JM: Yes. Yes, and also some people came for sabbatical. They might not be part of Szent-Györgyi's lab in particular, but they were fellow scientists with whom one might exchange ideas. I think that those people would perhaps not have come on their sabbaticals if there had been some year-round activity at the MBL.

AK: Right, and he was the year-round activity for a long time.

JM: Yes, and then NIH set up a lab up here and that would have been maybe in the mid to late '70s.

AK: Uh-hmm, is that still the case? NIH still . . .

JM: Well, yes, several investigators are funded by NIH.

AK: Uh-hmm. Delbert Philpott, I spoke with out in San Jose a few months back. Delightful guy.

JM: Yes.

AK: He told me an interesting story having to do with the year-round aspects. Evidently one of the arrangements having to do with the electron microscope was that Szent-Györgyi's lab could use the electron microscope most of the year, provided that they maintained it—actually Delbert Philpott maintained it and kept it going and ran other people's work on it during the summer months. There was kind of a tradeoff there.

JM: Yes. Well, that was the first electron microscope, of course, that came into the MBL and this was a time when that was sort of the thing to have and for the MBL to have acquired one within this arrangement with Szent-Györgyi was a big plus for the MBL in general and it was extraordinarily useful to Szent-Györgyi lab people.

AK: Did you use it?

JM: I didn't use the electron microscope, no. The muscle people were mostly using that.

AK: Can you think of any other similar arrangements that in a sense took advantage of the year-round versus the summer months phenomenon?

JM: I don't think of anything immediately, but maybe something will come to mind if we move along.

AK: Okay. Well, you've actually already mentioned one, now that I think about it, and that is that for scientists on sabbatical, it was useful to have a laboratory here that operated year-round, and maybe even just as useful that Szent-Györgyi lived here year-round because they could meet at his house. Part of Szent-Györgyi's thinking when he purchased Seven Winds, his house here, was that it might be a place that would attract visiting scientists.

JM: Yes.

AK: And it did, evidently.

JM: Yes, it certainly did and we often had seminars at Penzance, on the south porch of Szent-Györgyi's house, and he liked to offer his summer cottage to selected people and being able to invite them as guests for a while in his summer cottage was sort of a substitute for having money. Then these seminars would often involve whoever was occupying the summer cottage, and that would be someone with a particular area of expertise. It made for very interesting times and information. Personalities were very interesting, too, of course.

AK: Tell me a little bit about one or two.

JM: Well, [George] Gamow.

AK: Gamow. This was the Big Bang Theory?

JM: Yes. He was sort of a party person himself. You're familiar with the party that Jim Watson sprang on him, are you?

AK: No, tell me. This is Watson of Watson and Crick?

JM: Yes. In 1954, a year after Watson, Crick had published their paper regarding the double helix, Jim Watson sent out invitations to just about everybody in Woods Hole, it seemed, to a party at Gamow's, meaning Szent-Györgyi's cottage, and wrote it up under the Mr. Tompkins idea—Gamow's math books were Mr. Tompkins story. They were rather interestingly written. But anyway, Gamow knew nothing about the party until the RSVP's began to come in, but Gamow decided to go along with it and stocked up on liquor and whatever. But he also made this model to depict Jim. He had a plexiglas piece. Actually, I think you have a picture in the collection because it was possibly taken by Del Philpott.

AK: Yes, I recall it.

JM: Where he had this spiral in the middle and then he had a couple of slinkies with big tennis balls and blue dots on them for eyes and a marine jaw, and Jim's slouch hat on top and his broken down sneakers underneath, and that sort of thing.

AK: When you say "he," this was Szent-Györgyi or Gamow?

JM: Gamow made it. But, Gamow had a good sense of humor and you probably are aware that he and Ralph Alpher were at Los Alamos together and were getting ready to publish a paper when Gamow thought it would be nice if he could have Alpher Bethe Gamow. So he got Hans Bethe to lend himself to this, and that paper was published under those three names in Nature. That gives you a sense of how Gamow was an interesting personality. [chuckles]

AK: Did he get along well with Szent-Györgyi?

JM: Oh, I think so, yes.

AK: Of course, Szent-Györgyi also had a tremendous sense of humor.

JM: Oh, yes.

AK: Sometimes when you put two people with great senses of humor together, you never quite know how it's going to work.

JM: Yes.

AK: But it worked.

JM: Yes . . . In terms of making use of the house, you know, and bringing people together, I think that Jim Watson wrote a good bit of one of his books at the Seven Winds.

AK: On the double helix?

JM: I think it was. I was looking at dates and I would have to look at dates again to comment on that, but it would have been something about '62 or earlier.

AK: You mentioned the seminars on his south porch.

JM: Yes . . .

AK: Now, he never sought an academic appointment in the United States, for reasons that you suggest had more to do with his aversion to committee work and his concern that administrative things would interfere with his research.

JM: Yes, Adrian, I think that is so. That was never expressed to me, but that's my feeling about it.

AK: Okay. In other words, he himself never said that to you.

JM: That's right.

AK: Did he ever say anything to you about academic appointments or his feelings about that?

JM: There was a time when Boston University was interested in appointing him, and I think he felt that it would be too much of a distraction. That was an area where Ben Kaminer would have been of assistance to you.

AK: Benjamin Kaminer was at Boston University.

JM: He was at Boston University, yes.

AK: Well, the fact that he didn't have an academic appointment does not mean that he was not a good teacher.

JM: Oh, he was an excellent teacher. An excellent lecturer.

AK: Tell me a bit about that. I know much of the teaching, of course, was informal in the sense that there would be daily gatherings in the laboratory and meetings in his house, but there were also some formal occasions. Can you tell me a bit about how often this occurred and where? Was it always here?

JM: Well, he was often an invited speaker, both nationally and internationally, and as for his speaking to our own lab group, that would depend on how things were evolving. In addition to that, he was always invited to speak for the physiology course at MBL in the summertime.

AK: Would this be a single invited lecture?

JM: Well, I think it was maybe just one in that instance, but there may have been others, too, that I've lost track of. He always did these things very energetically, bouncing around on the stage and he could make his point.

AK: A natural teacher.

JM: Yes.

AK: Not only in the one-on-one sense, but also before an audience. He was not embarrassed to appear before an audience.

JM: Yes. In terms of the day-to-day basis, he would always arrive enthusiastically in the morning and we would have discussion about any new thoughts that came up over night and what we might think about that day. But then you didn't fuss with him. He wanted to concentrate at his desk by himself and those around him, and I speak particularly for

myself, wanted their own things in the same manner, with not a lot of hubbub around any particular experiment. You know, you might gather together around some sorts of things, but mostly the idea was to be able to focus and think and be quiet.

AK: I want to come back to what the work in the laboratory was like, but before we leave the subject of teaching, I'd like to ask you one of these impossible questions. I don't know how you're going to answer it, but I'll let you decide. I want to ask you, how you experienced him as a teacher. What did you learn from him? What and how did he teach you? Now, that's a hard question because you were with him so long.

JM: Well, he would demonstrate. He would demonstrate things that he was doing, and I would learn from that. Now, a lot of the things that he looked at included dyes and color changes and things of that nature. He could point out the subtleties. So I think I learned a few of those tricks and how to gauge subtle changes versus no change or great changes.

AK: He evidently was very good. He almost had an intuitive feeling for the way things were going to react.

JM: Yes.

AK: So he would teach by demonstration? By showing you?

JM: Yes.

AK: Often the reactions in the test tube would have color aspects. It was a very visual image way of demonstrating what was going on at the molecular or sub-molecular level.

JM: Uh-hmm. Uh-hmm.

AK: Was he a patient teacher? You smile.

JM: I'm actually trying to think one way or the other. I'm trying to think of examples. I would say he was patient.

AK: He was a patient teacher?

JM: Yes, yes. He would rather that one take time and be careful. He would not be impatient about conclusions, while you were still quietly going at something. But I can't think of any instance where he would be impatient.

AK: Well, one possibility would be if he were explaining something or demonstrating something and the other person didn't understand it.

JM: Well, it's probably a credit to him that he didn't have to be impatient with the other person, that he did it so clearly that it wasn't difficult for the other person to understand, at least in my experience.

AK: Right. So that is a sign of a gifted teacher.

JM: Yes. And I would expect that the more senior people in the lab probably experienced the same sort of thing.

AK: You mentioned the workman-like, focused atmosphere in the lab, after the initial discussion of "What are we going to today?" He'd share his ideas and then people would get down to work.

JM: Yes.

AK: Was there a break in the day? Did you all stop for lunch?

JM: We did stop for lunch at some point. It was not a definite time or anything, but occurred according to the convenience of the work. But for many years we had afternoon tea, and the women in the lab would take turns making the tea. Somebody would always see that there were cookies or something to nibble on with the tea, and everyone in the lab would come together. We would use an empty room during the off season. We didn't have this

in the summertime. We did it in the off season. Everybody would gather and we would cover the waterfront, really, in terms of subjects. A good bit of it could be science. Some of it was politics or any other interesting things. But it was about twenty minutes, twenty to thirty minutes for sociability, basically.

AK: He didn't much care for hierarchy.

JM: I think that's true.

AK: Help me work my way through this thought. He didn't much like bureaucratic hierarchy.

JM: That would be more to the point, yes.

AK: But in his laboratory, was there a hierarchy there in some sense? I mean, it was his lab.

JM: No, I don't think so. He wasn't concerned with—an image just popped in my mind, but he wasn't concerned with anything fancy about the lab. For a long time, he had a wooden table for a desk that looked like somebody had put it together for working on outdoors or something. There was a point at which the MBL shared a secretary with him and the secretary was very interested in having a decent desk, but it was in his lab, you see. Her desk was on the first floor, it wasn't in the lab. I think she finally persuaded him to get something a little more desk-like than the wobbly table that he had been working on. The

MBL much preferred his having the secretary do his letters because he would type a letter and it would be running off the end of the page. The typos would be all over the place. You could tell it was his, simply because of the way it looked.

There was a point in the '70s I think when he was doing—he generally did his own letters at home, except for this period when the secretary was offered to him, but he had sent a letter to a couple of associates within the National Foundation for Cancer Research (NFCR), one in London and one in Austria, I believe. He wanted to see them about something, so they flew over and they said to me, "Jane, did you prepare that letter for him?" and I said, "No, I never do that." His wife had written the letter and they weren't sure it was really from Prof because it didn't have this beautiful pattern of things that he would put into a letter—typographical errors, no margins, etcetera.

AK: [laughs] Now, if he shared a secretary with the MBL, I suppose then that maybe the National Foundation would have paid part of that salary. Was that part of the . . .

JM: Well, this is—what I'm thinking of is prior to NFCR.

AK: Prior to that?

JM: Yes.

AK: So some other funding that would have paid partially.

JM: Yes.

AK: Part of Szent-Györgyi's commitment to science was his idealism about the work, almost as a calling, that one should be prepared to make considerable sacrifices for.

JM: Yes.

AK: In fact, he—this was in 1943, he wrote what I'm about to say and of course one has to think that the war was on, so one has to put it in that context, but he says that "Scientific research is a passion. The real scientist is driven by this passion and he's ready to bear privation, of if need be starvation," which of course he did over there, "rather than let anyone dictate to him which direction his work must take." That seems so characteristic of what he . . .

JM: Yes.

AK: Now, on the one hand that's very idealistic and commendable. On the other hand, and this is what I want to ask you, it sets a very high standard of personal commitment to the work.

JM: Well, it is and one of the advantages of living right in the village where the lab is would certainly lend itself to accommodating that, in that you almost invariably had things to check at night or during the weekend and it would make it easier to not have to commute to do it.

AK: You lived at Woods Hole.

JM: Right, I've lived right in Woods Hole, since I came here.

AK: Did you take vacations?

JM: I did.

AK: You were ready, I take it, to make a strong commitment to the work.

JM: Well, when he first offered me the two-year fellowship, he said, "Now, if you accept this," he said, "I don't want you changing your mind in six months or a year or so." He said, "A definite two years, and then we'll see what happens after that." At that time, given the population of this area, I thought to myself, "Two years?" and the two years turned into thirty-four in that particular lab, and I'm still here. It was the right thing for me.

AK: Scientific research, what's going on in the laboratory, doesn't know that it's a weekend.

JM: That's right.

AK: It doesn't care about the human diurnal cycle. So in a sense scientists often do have to, as you say, be there and be ready to take care of things.

JM: You have to be prepared to look after things at any hour of the day or night, you know.

AK: Yes.

JM: You know, that sounds like too much when I express it, but the interest and commitment was there. There were periods when one didn't have to do that.

AK: Okay. So it was a commitment, but it wasn't oppressive?

JM: No.

AK: But it did help that you were here on site.

JM: Yes.

AK: Okay. I'd like to ask a little bit about your job. We're sort of in that area now. I'm from Washington, DC, so I'm used to this term, "What's your job description?," you know.

JM: Oh, yes.

AK: And I know that there's always a formal job description, and then there's the real job.

JM: I don't know whether there was ever a formal job description.

AK: [laughs] What were you hired to do and what did you end up doing? Were they the same?

JM: Well, in accepting the fellowship, I came in as Szent-Györgyi's assistant, shall we say. In that regard, I was doing the things that any assistant would be doing. And as things evolved, he would refer to me as his associate, but there were never any formal mentions of these things.

AK: Right.

JM: But among the things that one did was not only the research in the lab, but the papers that were published and so on. He wrote several books over the years and so editing was probably one of the things that fell into my general description.

AK: You also were co-author on many articles.

JM: Yes. Both with Dr. Szent-Györgyi and with some of our other colleagues. Sometimes those papers were such that Szent-Györgyi wasn't actively involved in them.

AK: In an informal, a relatively informal, work setting, such as Szent-Györgyi's lab, which is sort of the opposite of a bureaucratic personnel department . . . it's nice in some ways to have that degree of informality and freedom. On the other hand, how does one negotiate things like salary increases?

JM: I don't think we ever had any discussion about that. But if there was money, there might be an increase.

AK: Was that ever an issue?

JM: It was never an issue, no.

AK: Did you feel that it was taken care of adequately?

JM: Well, I always felt I could use a little more, but everybody was in the same fix, you see. The economy in the '50s and into the '60s was not that good, but you could accomplish things with a lot less money. We adapted.

AK: Right. Things like the parties at his house or the volley ball games or any number of things that went on, you would participate in organizing those or you would participate in them?

JM: Well, most of those occurred in the period up until the time when Marta died.

AK: Which was about '63.

JM: '63 when she died. She was quite a fine hostess and a good organizer. The gatherings, the party end of things were usually costume parties. Marta would say, "Well, if I'm going to work to put this party on, those who come are going to have to work to come as something." So they were fun.

AK: Did that not happen then so much after 1963?

JM: A little bit, but not as it had in the earlier days. I think there were a few, but not noticeable.

AK: Right. Did that make a big difference, her death? When Marta died did things changed?

JM: Oh, it made a tremendous difference to him, yes.

AK: Made a tremendous difference to him.

JM: That was a very, very sad ending of things.

AK: Yes. He was here for twenty-three years after that.

JM: Uh-hmm.

AK: Were the twenty-three years after that, then, different?

JM: Well, they were different in that concurrently with that or around the same time a lot of the more senior fellows were getting into academic situations. So the numbers in the lab changed, the personnel changed.

AK: Did you yourself ever think about a different position?

JM: Well, there were times when I thought that I was not sure what was going to be happening and I should be thinking about it, but I wanted to continue if possible. Then following that I was able to be associated with others in the MBL between '86 and '02.

AK: Right. Now, in some ways Szent-Györgyi was a person of habit. He would always come in the morning. He would be enthusiastic. He would discuss things. He would settle

down to work. There might be a tea break. There was a certain rhythm to the day, to the week.

JM: Yes.

AK: Did he ever surprise you?

JM: Did he ever?

AK: Surprise you? Did he ever do something you hadn't expected?

JM: Not that I recall.

AK: You knew him as well as anyone, I'm sure.

JM: Yes. I don't think of anything that surprised me very much. No.

AK: It's interesting, the combination of a very creative original thinker and a person of habits. In a sense, you don't know what he's going to think, but you always had a sense you knew what he was going to do.

JM: Once you have a sense of the person, then you know what you can count on—I'm certainly not speaking on the scientific thinking when I respond to that question.

AK: I understand, yes. But you could count on—you could count on him. His rhythms, his styles.

JM: Yes.

AK: He wasn't a particularly moody, quixotic . . .

JM: No, no.

AK: When he came here in '47, having won the Nobel Prize ten years earlier, he was already an accomplished scientist in midlife. He found a place here in the United States. But what aspect of his work as a scientist in this country do you think was most difficult for him?

JM: I think that not having better funding was probably the biggest challenge and maybe disappointment.

AK: Uh-huh. He hadn't expected that it would be so difficult?

JM: Well, I don't know but I think he perhaps felt that with his track record that it should be a little bit easier to have the funding. At times when things got very tight, then there would be some wealthy patron who would come along and help him over the period.

AK: So it sounds as though the most difficult aspect was this whole competitive "what have you done for me lately" grant application procedure.

JM: That sort of thing.

AK: Andrew said that he thought that Albert was "constitutionally unable" to apply for grants.

JM: Well, I think that would be something that he would not be patient about.

AK: Yes, okay.

JM: But Andrew could give you a better fix on that.

AK: Yes. Let me ask the opposite of that question. What about his work as a scientist do you think gave him the greatest satisfaction?

JM: Well, I think being able to continue to work all of the time, and to go forward with his thinking while at the same time recognizing that some of his ideas had not been correct.

He'd be the first to admit that, but he was always feeling that he was moving forward with his thinking. I don't think it has been recognized yet. You know, I think it's still a question out there and it will be interesting to see what the conclusion is ultimately about his work.

AK: Tell me a little more about that. What's the open question?

JM: Well, I think perhaps where quantum [theory] fits into the whole picture, the sub-molecular picture. I can't express it more fully than that.

AK: Yes. So it remains to be seen, perhaps, how prescient he may have been in thinking that there was a lot to be gained from analyzing life at the sub-molecular level.

JM: Yes.

AK: I remember reading something he said once about how the test of a theory is how many useful experiments it can generate. You can tell a bad theory because people keep building more and more aspects of the theory to cover up its errors. He was very intellectually honest, wasn't he?

JM: Yes, yes.

AK: As you say, he would be the first to say that something he was pursuing was not working.

JM: Yes, that's right, but at the same time the sort of "big hook" type works were generating ideas amongst others that was possibly very fruitful. You know, the thinking got broader and this is a reason why he welcomed Gamow because Gamow was mathematical, along with the physics and everything else. And people like the Pullmans, Bernard and Alberte Pullman. You expressed it in what you said a few minutes ago that even the mistaken theory generates a lot of thinking and some good and some not so good in their results.

AK: In a sense this is related to the frustration he had with the grant process because it doesn't seem to me that the grant funding process is oriented toward these kind of "big hook" issues. Was there a time in his nearly forty years here at Woods Hole that—well, I'm going to ask you both sides of the same question. Was there a period here that you think might have been regarded as the darkest period for him, and then what do you think were the golden years or the best years?

JM: Well, I think the darker years were the years following Marta's death. And . . . you're thinking golden years scientifically?

AK: It's hard to separate, I understand. What I had in mind was an image where everybody's busy, everybody's happy, and both of them happen together.

JM: Uh-hmm. Well, I think the early '60s, late '50s early '60s would probably be considered good years. That's not to exclude others, but I'm just trying to think of patterns that occurred.

AK: Yes. Since you were with him for thirty-four years, I'm making an assumption that you had a chance to observe developments, the evolution of his thinking over that period of time. I think it's a safe assumption.

JM: It is.

AK: Because not only were you working with him, but you published things with him over the years, and people who worked in the lab spoke with each other so frequently about what they were doing. There were some constants in those thirty-four years. Some things stayed the same. He was always interested in, I think, the basic questions about life. How does it work? How do living cells work?

JM: Yes.

AK: That was a constant. How he chose to investigate that question did change over the years, I think, did it?

JM: Yes, it did.

AK: Tell me how you would describe some of the ways his thinking, his approach, the questions that were on his mind, how those things evolved over time. One of the ways it has been characterized has been, you know, vitamin C, muscle, quantum. I don't know if that's fair.

JM: Well, there's an inclination for people to describe his Nobel Award in terms of vitamin C, but actually it was for his oxidation reduction study, and it's the oxidation reduction element that plays through everything, I think.

AK: Tell me more about that. This was the work that got him on the verge of figuring out what then became known as the Krebs Cycle.

JM: Yes. You see, it just is simpler for people to express the Nobel Award in terms of vitamin C and let it go at that, but if you look at the record, you'll find that it's expressed as I would say primarily for the oxidation reduction studies and then this other— recognition of vitamin C. A lot of the movement forward might also be related to development in equipment that could be used to look at things in a different way. When Irv Isenberg joined the lab, he had this large magnet, the Electron Spin Resonance equipment, that made it possible to look into different aspects of the question. When Ron Pethig and his students came, they did electron spin resonance studies, and so it is like treating a medical problem, you see. When something new opens up, then you can begin to handle it and think about it in a different way. But it was not the evolving of scientific

equipment that contributed to the directions that the investigators took but rather the scientists' need that led to the equipment being developed. However, Szent-Györgyi always liked to work very simply at his bench. You know, let the experts use this heavy equipment, but the things that were being studied with the heavy equipment were things that came from his observations at the bench with the simpler test tubes and solutions and so on.

AK: Right. For example, you mentioned Ron Pethig and the electron spin resonance technology that allowed for some quantification of electron activity that Prof Szent-Györgyi had thought about at the bench?

JM: Yes.

AK: But here's a way to measure it.

JM: Yes.

AK: Now, he did use that technology, did he?

JM: Yes.

AK: You're saying he didn't get hands-on involved in the machinery.

JM: Right.

AK: But people who worked with him did.

JM: Yes.

AK: Okay. So then a lot of the evolution of his thinking came out of the original work involving the oxidation reduction data, the ideas that he got that were involved actually in the Nobel Prize. It wasn't, as you say, just vitamin C.

JM: Yes.

AK: Okay, and what helped it evolve was a combination of his continued thinking and the evolution of new technologies to test what he was thinking.

JM: Yes.

AK: Okay. Do you think people appreciated—well let me give you the idea that came to my head. You can tell me why it's wrong. I think it might be wrong. The idea that came to my head is later in life, when he would make demonstrations with test tubes changing colors, did this strike people as anachronistic?

JM: I don't know. I'm thinking about lectures that he gave when he was demonstrating this and my feeling was it was well received and well received because they knew that it was his style. But as we're talking, Adrian, it strikes me that it helps to live a long time to be able to grow with the evolution of equipment to prove or disprove your ideas, your simpler ideas.

AK: If one is open to it, and he was.

JM: Yes.

AK: Do you think his thinking was pretty clear and sharp through his eighties?

JM: Oh, yes. Yes.

AK: That in itself is pretty remarkable.

JM: And his nineties. He was ninety-three when he died. During his last weeks in 1986, I recall times when Ron Pethig, Peter Gascoyne, and I would meet him at home to keep him informed of what was happening in the lab—he would discuss the work and when he was satisfied that we needed no further discussions he would "shoo" us back to the lab—didn't waste time talking further.

AK: That's remarkable.

JM: I mean, it was clear until the end.

AK: Well, we talked about this constant in the evolution of his thought. You never saw then a dramatic change, a "paradigm shift" where all of a sudden he's on a different track.

JM: Well, he could have some very different ideas over night about things, but I don't recall any of them being on a completely different track. It was that those things sort of built up, built to a certain stage.

AK: You mentioned the big hook. This of course refers to his fishing.

JM: Fishing. More fun not to catch a big fish, than not to catch a little fish.

AK: So you may as well use a big hook.

JM: Yes.

AK: [laughs] These were the big ideas that he was pursuing.

JM: Yes. Yes.

AK: You said that this was a particularly productive way to lead a laboratory in the sense that from these big fish, when they were caught, so many ideas spun off for other people to pursue.

JM: Yes. Big fish could feed a lot of people.

AK: Could feed a lot of people. Good way to put it. One could say that in his long career, he made two kinds of contributions. I'll pose this as a hypothesis. You can tell me what you think. One contribution one could refer to as the style of his work, the way he worked. The other contribution was the actual content, what it was that he discovered. Which do you think would be his greatest legacy?

JM: I think the broadness of his thinking, and as I say, it's still a question what . . .

AK: Right. How did the broadness of his thinking constitute such a contribution? What came from it?

JM: Well, I think because of the thinking that it generated in different disciplines. You know, the quantum biologists and the physicists and different areas of science that were touched by this. I haven't ever studied what others have put together in connection with this, but that's what I think. I just think that the broadness touched many disciplines and while I

can't think of any specific items that were positive out of this, I can't believe that there weren't some or many that were good.

AK: So one contribution would be the fact that his ideas transcended disciplinary divisions, caused people to think or to pursue lines of research beyond the strict boundaries of molecular biology versus physical chemistry versus quantum physics.

JM: Right.

AK: All of these things were pulled together.

JM: Yes, yes.

AK: I suppose one aspect of that is that many different people from these disciplines got involved in and inspired by his thinking. Ron Pethig is probably a good example. This volume by Szent-Györgyi, *Introduction to Sub-Molecular Biology*, inspired him. He said it was as though Szent-Györgyi had read his mind.

JM: Yes.

AK: Ralph Moss in his biography of Szent-Györgyi wrote this about you, and I'd like you to respond to it. What you think about it. "Albert himself was so given to flights of fancy

that for him to do productive work, he needed more stable collaborators. He always needed someone rock solid, off whom to bounce ideas. From the early 1950s this role in Albert's life was played by Jane McLaughlin." What do you think of that characterization of your role in his life?

JM: I know that at one time, I can't remember just what this is linked with, but he described me as "his faithful critic."

AK: You were his faithful critic.

JM: Yes, whatever that means. But I really don't know how to interpret Moss's comment. You know, I just did research as I thought best and Szent-Györgyi did things in his own style.

AK: Faithful critic, that's an interesting idea. It means that he could trust you to be objective without being disloyal.

JM: Well, yes. I think so. I just offer that to you as a comment that Szent-Györgyi injected at some point in time.

AK: What do you think he meant by that?

JM: Well, it could relate to just about anything, you know, whether it was politics or science or anything else. But to me it sounded too much of a label to put on me, you know.

AK: Faithful critic?

JM: Yes. You see, Adrian, I'm just a simple everyday person and I don't put labels on people and certainly not myself. I would do things that I was able to do. I must say for someone who did not have an advanced degree that I certainly always had the respect of everybody around me, and it didn't matter, my lack of doctorate. See, in some situations there would be a big difference in how one might be treated on the degree level, and I never experienced that. I would say that I was always treated very positively.

AK: Uh-hmm. I don't want to get too much off the track of science, but he did get involved in political activity. He had strong opinions about social change.

JM: Uh-mm.

AK: Of course, the '60s and '70s were . . .

JM: Oh, they were wild.

AK: Rather tumultuous, indeed. Did you talk with him about such things?

JM: We did talk about some things and I was certainly more conservative than he was about them. But we didn't have any conflicts between us. Everybody respected everybody else's ideas and thinking and reactions.

AK: So you could disagree, as they say, without being disagreeable.

JM: At times, I could disagree by not saying anything, you see. He would know that if I said nothing, that was a message.

AK: I see.

JM: He did comment on that one time. "You haven't said anything, but I know what you mean." [chuckles]

AK: Okay. Is there anything that we haven't touched upon? Any anecdote? Any thoughts you have about your years of working with him? Anything about him that you would like to say before we finish up?

JM: Simply to say that I certainly enjoyed the many years of my association with him.

AK: What did you enjoy?

JM: Well I enjoyed actually doing the work and I enjoyed the colleagues. Most of them I've kept in touch with all of these years, those that are still around. You know, it was a nice—it was a community here, of thinking, and I enjoyed learning lots of new things. Constantly learning new things. Pulling one's thoughts together to express in a report or a paper for publication, that sort of thing. You know, I was growing up through all of this. This is what it amounts to, scientifically.

AK: You had a good scientific career here at Woods Hole.

JM: Oh, I think so. Yes.

AK: Thank you very much.

JM: Well, thank you. I'll look for the written version.

AK: Okay, great. Thanks.

[End of Interview]

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